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Modern Social Conditions. A Statistical Study of Birth, Marriage, Divorce, Death, Disease, Suicide, Immigration, *etc.*, with special reference to the United States. By WILLIAM B. BAILEY, Assistant Professor of Political Economy in Yale University. New York, The Century Company, 1906.—377 pp.

The nature and scope of this book would have been more clearly indicated by its title had it been called "Demography of the United States" or "Vital Statistics of the United States." Its object is "to meet the needs of American students who are interested in the structure and growth of the population of their country," and the effort has been to make it serviceable both as a text-book and "as a book of reference for those desiring information upon the topics covered" (preface). It contains six chapters entitled, History and Theory of Statistics; Sex, Age and Conjugal Condition; Births; Marriage; Death; The Growth of Population. The book invites comparison with Richmond Mayo-Smith's *Statistics and Sociology*, to which the author frankly refers. He writes:

> Since the appearance of this work so much has been done by the scientific world that this seems a good time to bring together part of the results of their investigations. Wherever possible the figures relating to the United States have been used, but those for foreign countries have been introduced for purposes of comparison and to illustrate points upon which the statistics of this country are lacking [preface].

The present work, however, is narrower in range than the earlier, embracing little or nothing upon crime (other than suicide), upon immigration or upon the dependent or defective classes. It is a compilation and interpretation of statistics, especially American statistics, derived from various sources.

In considering the merits of such a book the questions which present themselves are: Is it based upon the best authorities? Are those authorities treated with judgment and acumen? Are the most important results which have been attained in each branch of the subject brought out clearly and in their proper relations?

The author's treatment of two important subjects, the death rate of the United States and the birth rate of the United States, has been examined with the foregoing questions in mind. Regarding the death rate of the United States he says: "The best that can be done is to give the figures for the registration area. The death rate for this area in 1890 was 19.6 and in 1900, 17.8" (p. 215). A foot-note states correctly what was included in the registration area in 1900. A comparison of this summary treatment with the paragraph given to the same question by Mayo-Smith (Statistics and Sociology, p. 148) reveals no advance in the later book over the earlier. Our author gives no hint that the registration area changed boundaries or content between 1890 and 1900; that Maine and Michigan were included only at the later date, and Delaware only at the earlier; that 153 cities outside of the registration states were included at the later date and only 83 at the earlier; and that, largely in consequence of these changes, the registration area of 1890 embraced 31 per cent, and that of 1900 38 per cent, of the country's population. There is no warning against an uncritical acceptance of the figures quoted as proof that the death rate of the United States in 1900 was much below that of 1890. The question whether the population of the registration area at either or both dates was representative of that of the whole country is ignored. So, too, is the crucial question: What is the probable error of these figures and in which direction from them does the truth probably lie?

In my judgment the most important authorities on the topic are the Twelfth Census; a detailed and severe criticism of the vital statistics of 1900, by F. L. Hoffman, in the Publications of the American Statistical Association; and a brilliant paper by Professor Irving Fisher, "Mortality Statistics of the United States Census," in the Publications of the American Economic Association. Of these authorities the author seems to have used only the first and that uncritically.

Regarding the birth rate in the United States Mayo-Smith made the following statement:

> The birth rate as calculated by the Census of 1890 was for the whole population 26.68; for whites, 26.35; for colored, 29.07; for whites, with both parents native, 28.58; with both parents foreign, 38.29. The last two figures were only for limited areas. These rates are probably too low by from 20 to 30 per cent, owing to deficiencies in the return of infants "born and died" during the year [p. 69, note].

There is hardly any branch of American vital statistics possessing greater popular and scientific interest than this, since it furnishes whatever trustworthy basis there is for the discussion of what has been called "race suicide"; and one turns to the book before us expecting to find a summary of the present state of knowledge and opinion regarding it. Yet the book contains no statement upon the probable birth rate of the United States and no explanation for its omission. The subject has been handled since 1890 by at least four competent statisticians who have expressed and defended in easily accessible places widely different opinions. To ignore the whole matter may have been the easiest treatment of it, but it cannot be called satisfactory.

The preceding criticisms are open to the reply that they merely point out omissions but throw no light upon the accuracy or importance of what is said. Although this reply would not meet my contention, which is that the most important topics have been ignored or slighted, still it is fair to probe also the merits of what the book contains.

For this purpose I have selected, by opening the book at random, two passages, pages 110 and 111, 174 and 175; and I have also taken, as an example of work in a widely different field, a third passage giving an epitome of the history of statistics, pages 5–7.

On page 110 this statement occurs: "The following table was presented by Dr. J. Bertillon before the meeting of the International Statistical Institute at St. Petersburg in 1897." The table contains an error in figures, the 80 in the first column is 79 in the original, and a more serious error in words, representing by "quarters" an idea which in the original is indicated by "*arrondissements*" for Paris, "*Stadttheile*" for Berlin, "*Bezirke*" for Vienna, and "districts" for London. The change from "districts" to "quarters" is misleading, because "quarters" is ambiguous and, when social position is in question, is naturally interpreted as referring to the housing of the families rather than to the part of the city in which the families dwell. That the writer fails to mention what is to me the most significant thing about Dr. Bertillon's article, namely, the various criteria by a combination of which he classifies the districts in these capitals according to the social position of their residents, may be a matter of individual judgment, but there can be no such answer to the criticism that he has fathered upon Dr. Bertillon a table which is not to be found in his paper. No doubt the material from which the table has been constructed is in the article, but the table is not. And in my judgment Dr. Bertillon would not be willing to stand as authority for it. Suppose that the election districts of New York City, Chicago and Philadelphia were classified according to the social position of their inhabitants, the criterion in New York being the proportion of families having at least two persons to a room, that in Chicago being the proportion of heads of families paying a state income tax, and that in Philadelphia being the average number of domestic servants in each hundred families, would a careful statistician like Dr. Bertillon assemble his results in one table? It would have been quite as easy to say, "The following table has been compiled from material presented by Dr. Bertillon," *etc.*, and the statement would then have been correct.

On page 111 a table is given showing the comparative birth rate in different districts of Amsterdam, the poorest districts having a rate of 118.2, or 18.2 per cent above the average for the whole city; the richest districts having a rate of 64.4, or 35.6 per cent below the average for the whole city. Then follows this comment: "We have here the curious coincidence that there are about three times as many births in the poorest as in the richest quarter." The most favorable explanation —that the sentence belongs after a preceding table—I have had to reject. The "curious coincidence" is with the results of that table. But there is no coincidence, since in Amsterdam there are less than twice as many births in the poorest as in the richest districts; and if there were a coincidence it would not be curious, because the criteria employed in classifying the districts were not the same for any two of the five cities and the lines of division between the several classes have to be drawn arbitrarily, so that, granting any difference at all to exist between them, the amount of it depends largely upon where the person who makes the table decides to draw the lines.

On page 174 is the statement that "in Prussia, 1889–1900, the legitimate fecundity of the families in independent circumstances was 5.09, while that of the employees was only 3.69. The reason for this was that in many cases, among the poor, children were born before marriage, and in this way the number born after marriage was reduced." The authority quoted uses in the text the word "*Hilfsarbeiter*," but in the table the words "*Gehilfen, Gesellen, etc.*" It is not clear that the latter has any better claim than the next class, "*Taglöhner*," to be translated "employees." In the light of the clear implication earlier in the same article (p. 522, f) that the Prussian statistics do not enable one to distinguish the legitimate fecundity of employers and employees in a given occupation, I do not think the words will bear the interpretation given them. But, waiving this point, the German author proceeds to suggest two possible explanations of the difference: first, that employees gradually rise into the rank of employers, and that employers, as a class, are consequently older, have been married longer and have acquired families more nearly of their final size; secondly, that among employees the proportion of illegitimate births is larger than among employers. No evidence is offered that either explanation is the true one. For this and other reasons the passage seems to me unimportant as well as a little obscure; but, however it be understood, it is certainly inadmissible to transform, as our author has done, one of two possible explanations into a reality and then to make it "the reason for" what is probably not a fact.

On page 175 a table is quoted from "Rept. State Board of Health, Connecticut, 1902, p. 260." Reference to the authority fails to reveal the table. Further search shows that two public documents of Connecticut, the report of the State Board of Health and the Registration Report of the Bureau of Vital Statistics, are bound in one volume but paged independently, and that the table in question is in the Registration Report for 1901. In copying the table the number 329 has become 392, and the number 21,525 has become 31,525—errors testifying not merely to careless proof-reading but also to the fact that the tables have not been verified by addition on the proof sheets, an indispensable requisite of accurate work in reproducing statistical tables. The second error is the more glaring because the table in its erroneous form contradicts the most important inference which the author draws from it.

In one respect Professor Bailey has gone out beyond the field occupied by the earlier book of Mayo-Smith. He has given a very few pages at the start to an epitome of the history of statistics. The main authorities upon which one should rely for such a sketch, arranged in the order of my preference, are: H. Westergaard, *Lehre von der Mortalität, etc.*, second edition, chapters 2 and 3 (written from the point of view of vital statistics by a master hand); Adolph Wagner's article "Statistik" in volume 10 of Bluntschli-Brater's *Staats-Wörterbuch;* V. John, *Geschichte der Statistik;* A. Gabaglio, *Statistica*, volume 1; A. Meitzen, *Geschichte, Theorie und Technik der Statistik*, part 1. Of these and other authorities only the last-named (which, with the exception of Wagner's brilliant article, is also the briefest) seems to have been used in the preparation of this epitome.

What has been the result? Muenster's name is printed Muenstef, and we are told that he "was the first to furnish a work giving statistics of various countries" (p. 5). As no definition of statistics has been offered, the reader is left to assume that it is used in its present meaning and would get no inkling that there is probably not one important statistical statement in Muenster's *Cosmographia*. The reason is that statistics as we understand them, that is, numerical statements regarding social groups, did not then exist. But was Muenster "the first"? I have in hand the *Asiæ Europæque Descriptio* of Aeneas Silvius, afterwards Pope Pius II, with the date 1531 on its title-page. The first part of Muenster's *Cosmographia* appeared in 1536. The earlier book is of the same general character as the later, but much smaller and briefer. I believe the author has been misled on this point by his main authority, Meitzen.

The next two sentences are: "It was reserved for Achenwall (1719–1772) to first use the word 'Statistik.' It comes from the Italian *statista* which meant about the same as our statist or statesman." A foot-note cites an article by John as authority. The pertinent statement in the article quoted runs as follows: "This professor . . . . is often regarded to-day as originator of the name statistics, although neither the name nor the study to which it is applied began with Achenwall. In 1672 appeared the *Microscopium statisticum* of Heleno Politano. . . . It was not even a service of Achenwall to unite the name with the study." Our author, therefore, lends his support to a position which his authority rejects and apparently disproves. Perhaps he had in mind the following statement of Meitzen, to which he does not refer: "In this introduction he [Achenwall] first used the word 'Statistik'" —a correct statement but misleading unless explained. Achenwall was the first writer on these subjects to use German and the first to translate the Latin phrase of his predecessor, Conring, *notitia rerum publicarum*, into German. In doing so he coined a German noun *Statistik* from the then common Italian word *statista* and the Latin adjective *statisticus*.

The few words given to Süssmilch follow the sentences on Achenwall and precede the sentences on John Graunt, which is as if one should mention Newton before Kepler in a sketch of the history of astronomy or Cortez before Columbus in a history of America. The whole account is made lame and faulty by the failure to recognize and emphasize the differences between the word "statistics" and the thing. The word first came into common use in Germany as the name of a descriptive political science without any numerical data. The thing originated in England with John Graunt, and it was baptized "political arithmetic" by his friend Petty. Süssmilch imported the English books and ideas into Germany and hailed Graunt as an intellectual Columbus. Gradually the English thing displaced the German and the German name displaced the English. This fundamental fact in the history of statistics is recognized by John, who divides his *Geschichte der Statistik* into two books: the first dealing with the history of German university statistics, the second dealing with English statistics or, as he calls it, statistics in the present sense of that term. The same distinction was earlier made in Adolph Wagner's article. He called the two currents the German school and statistics proper. But the true course for one concerned with the history of vital statistics is the one followed by Westergaard, who ignored the whole development of German university statistics prior to 1800 as of interest only to antiquarians.

I am compelled to conclude that the book is not based upon the best authorities, that the authorities followed have not been used critically, and that it is not an adequate presentation of the present condition of American vital statistics.

WALTER F. WILLCOX.

CORNELL UNIVERSITY.

Women's Work and Wages. By EDWARD CADBURY, M. CECIL MATHESON and GEORGE SHANN. London, Fisher Unwin, 1906.—368 pp.

This admirable volume presents the results of the work of several years, performed by three competent persons. It is an enlightened study of the conditions of labor among women and girls in the city of Birmingham, England, covering more than one hundred trades and occupations. The authors say, in their introduction:

> The present enquiry has aimed at systematizing and offering in comprehensive form, for the purposes of the general reader and the social worker amongst women, the valuable work recently done by various writers and associations, and the present writers have taken full and free advantage of the work done in this field. At the same time a great deal of original investigation, extending over a period of three and a half years, has been done, and we offer no conclusions except those which our own enquiry bears out.

The conclusions are valuable for students in America, perhaps, quite as much as for English social workers. First among them is that little can be expected from trade-unionism in the near future for the lowest class of women workers. This, however, is carefully qualified by the further observation:

> While accepting this conclusion in relation to this particular class of workers, the results so far attained show that much might be done amongst the better class of women, especially amongst such workers as shop assistants, clerks and teachers, where men and women often do much the same quality and quantity of work.

For American readers it is difficult to conceive an industrial nation in which the limits of the hours of labor of women workers in manufacture (aside from the home industries) have been legally established so long that they do not form a subject of discussion. The principle that the legal working day for women can and must be regulated by Parliament was accepted in 1842 and has been applied in ever-widen-